

## Reply

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### 1. The critical Richardson number issue

The Mellor–Yamada (MY) models (Mellor and Yamada 1982, hereafter MY82) underestimated the correct value of the critical Richardson number  $Ri_c$  above which turbulence becomes inoperative. Specifically,

$$\text{MY82: } Ri_c = 0.2, \quad (1a)$$

while,

$$\text{Data: } Ri_c = O(1). \quad (1b)$$

Hassid and Galperin (2004, hereafter HG) call the MY value (1a) “somewhat low” while most people would consider a factor of 5 discrepancy serious. The discrepancy between (1a) and (1b) is also surprising if one considers that 13 years before MY82, Woods (1969) presented a lucid argument to conclude that

$$Ri_c = O(1). \quad (1c)$$

Woods made a clear distinction between the transition from laminar to turbulent flow at  $Ri_c = 1/4$  and the *reverse transition*, from turbulent to laminar flow (of interest here) that occurs at  $Ri_c \sim 1$ . Woods’ conclusion (see his Fig. 3) reads “since the final thickness of the unstable layer is nearly four times the value prior to the instability,” so is the corresponding value of  $Ri_c$ , namely (1c).

Three years after MY82, Martin (1985) showed that MY82 predicted too shallow ocean mixed layers, and that, in order to reproduce the measured values,  $Ri_c$  had to be pushed to  $Ri_c \sim 1$ , in agreement with Woods’ result (1c). Thus, before and after MY82, theoretical and empirical evidence was available that a reliable mixing model had better produce (1b,c). Cheng et al. (2002, hereafter CCH) solved the MY82 400% discrepancy and obtained

$$Ri_c = O(1), \quad (1d)$$

and yet HG write that CCH “is not necessarily superior

to” MY. We leave it to the reader to judge. In addition, HG make no mention of the fact that the low value of  $Ri_c$  prompted several people to abandon the MY82-like models in favor of other models that yield (1b) rather than (1a). Large et al. (1994) suggested a new mixing model, the KPP model, whose principal motivation was the fact that MY82 underestimated the ocean mixed layer depth. Thus, either one amended the MY82 model to recover (1b), as CCH did, or, as Large et al. did, one devised a new model not based on turbulence closure but that also satisfies (1b). Had the experts of the MY82 model heeded the warning of those who spotted the shortcoming of MY82 on the predicted  $Ri_c$  and corrected it, an improved MY82 would be widely used today.

CCH succeeded in reproducing (1b) within the general framework first employed by MY but HG now suggest to undo the success of CCH and bring the MY-type model back to a value of  $Ri_c$  less than unity, against all evidence accumulated over the years. As we now show, HG’s suggestion is based on several errors.

### 2. The length scale issue

It is imperative to clarify a key point that was misinterpreted by HG: in CCH, *no length scale of any type was needed to derive the value of  $Ri_c$* . In fact, no length scale enters in the solution of the  $P = \epsilon$  equation, which entails only the variable  $\tau$ , not  $e$  nor  $\epsilon$  separately, but only their ratio  $\tau = 2e/\epsilon$ , as Eq. (23b) of CCH explicitly shows, where only  $G_m \equiv (\tau S)^2$  enters versus  $Ri$ .

The solution of  $P = \epsilon$  is therefore the function

$$\tau S = f(Ri). \quad (1e)$$

Figures 1–8 of CCH do not require and thus did not employ any length scale. As we explicitly state in section 7, point 2 below, a specific expression for a length scale, Eq. (5e) of CCH, enters only in the construction of Figs. 9–11 of CCH. To clarify the issue let us use the general expression for  $K_\alpha$  (where the subscript  $\alpha$  stands for momentum and heat):

$$K_\alpha = c \ell^2 \tau^{-1} S_\alpha, \quad (1f)$$

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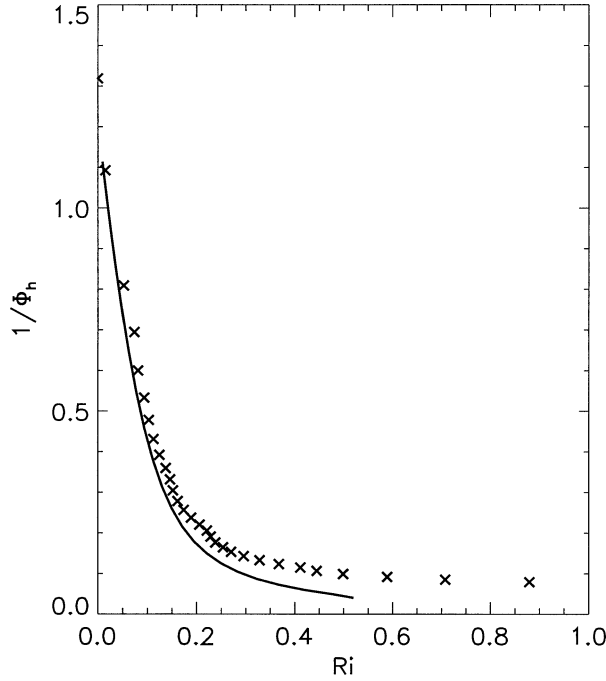


FIG. 1. Plot of  $\Phi_h^{-1}$  vs  $Ri$  (solid line) as from the HG suggestion, scheme 1 in section 2, as compared with the LES data (crosses) of Kosovic and Currey (2002).

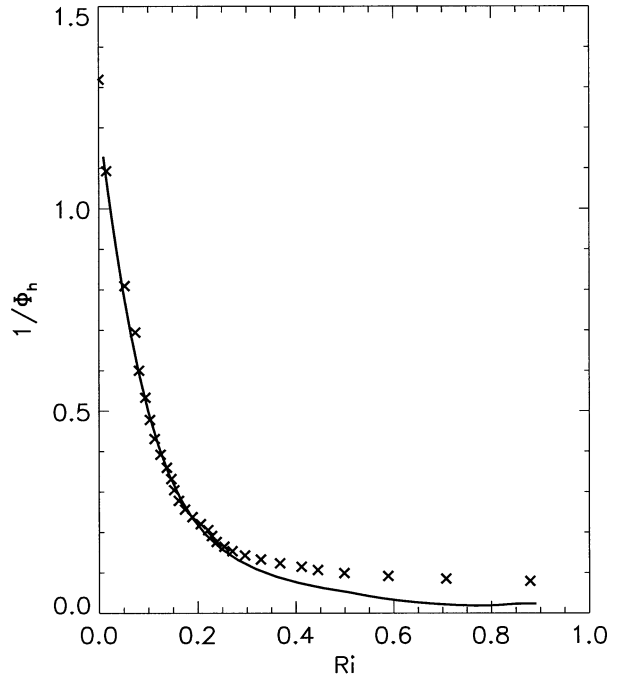


FIG. 2. The same as Fig. 1, but with the new scheme 2 discussed in section 2.

where  $2c \equiv B_1^2$  and  $\epsilon = (2e)^{3/2}/B_1\ell$ . There are three schemes to treat the variables  $\tau$  and  $\ell$ .

- 1) HG suggest that  $\tau(Ri)$  be obtained from  $P = \epsilon$  modified as per Eq. (1i) below. Furthermore,  $\ell$  is from Deardorff Eq. (5e) of CCH. The results for  $\Phi_h^{-1}$  are shown in Fig. 1 in order to show that it terminates quite abruptly at about  $Ri_c = 0.5$  while the large eddy simulation (LES) data are seen to extend much further. Clearly, the GH suggestion leads to a discontinuous  $\Phi_h^{-1}$ , a behavior that we deem unphysical. Note that this is the same as Fig. 10 of CCH, extended in the  $Ri$  axis to exhibit the abrupt behavior of the model  $\Phi_h^{-1}$ .
- 2) We put forward a new proposal:  $\tau(Ri)$  is from  $P = \epsilon$  unchanged, but  $\ell$  is treated as follows. Equation (5e) of CCH is implemented as follows:

$$\ell = \min(\ell_1, 0.53q_1/N). \quad (1g)$$

Here,  $q_1 = B_1\ell_1/\tau$ , where  $\ell_1$  is the length scale without the effect of stratification. This new procedure modifies both  $\ell$  and  $q$  but not their ratio  $\tau$ :

$$q = q_1\ell/\ell_1 = B_1\ell/\tau. \quad (1h)$$

This argument is hinted by Deardorff's original work (Deardorff 1976), which we understand as actually intending to limit, in his own words, the "mixing length" but not  $\tau$ .

The new results for  $\Phi_h^{-1}$  are exhibited in Fig. 2. Several points must be noted. Up to  $Ri \sim 0.5$ , the result does not differ significantly from that in Fig.

1. However, the sudden discontinuity in Fig. 1 is no longer present since the model now allows  $Ri_c$  to be of order unity. Thus, two advantages ensue: the maintenance of the  $Ri_c \sim 1$ , as we have already discussed and a more physical overall behavior of  $\Phi_h$ .

- 3) As CCH suggested,  $\tau(Ri)$  is obtained from  $P = \epsilon$  (unchanged), while  $\ell$  is from the Cheng and Canuto (1994) two-point closure model. The model is able to recover Deardorff (1980) and Hunt et al. (1988) models for  $\ell$  as particular cases. This  $\tau$ - $\ell$  scheme has not yet been implemented.

In summary, the HG scheme leads to a discontinuous result, Fig. 1, while our scheme does not, Fig. 2.

### 3. Hassid–Galperin's key argument

Hassid and Galperin's (2004) main contention is that, in deriving Eq. (1d), CCH did not take into account two limitations on  $G_h \equiv (\tau N)^2$  and  $G_m \equiv (\tau S)^2$ :

$$G_h < G_h(\max), \quad G_m < G_m(\max), \quad (1i)$$

which would change (1d) into

$$Ri_c = 0.52. \quad (1j)$$

We show that the first limitation in (1i) is due to HG's misreading of the physical basis of the Deardorff's limitation and that the second limitation in (1i) was explicitly shown to be satisfied by the CCH model.

#### 4. Use of Deardorff limitation

We begin with the relation

$$\text{Production} = \text{dissipation}, \quad P = \epsilon. \quad (2a)$$

Since both shear and buoyancy contribute to  $P$ , we write

$$P = P_s + P_b. \quad (2b)$$

With

$$P_s = K_m S^2, \quad P_b = -K_h N^2, \quad \text{and} \quad (2c)$$

$$K_{m,h} = 2e^2 \epsilon^{-1} S_{m,h}, \quad (2d)$$

where  $S_{m,h}$  are dimensionless structure functions, Eq. (2a) becomes the relation ( $\tau = 2e/\epsilon$ ):

$$\tau = \tau(\text{Ri}). \quad (2e)$$

On physical grounds, one expects that when  $\text{Ri} \rightarrow \text{Ri}_c$  and turbulence subsides, Eq. (2e) yields a very large lifetime since turbulence is weak and the nonlinear interactions no longer break up the large structures, which become (stable) linear structures, that is, with very large lifetimes. That is exactly what Eq. (2e) gives.

Notwithstanding this point, HG suggest that the solutions (2e) ought to be limited by the relation

$$\tau < \tau(\text{max}), \quad G_h < G_h(\text{max}), \quad (3a)$$

which they derive invoking the *Deardorff's limitation*: in a stably stratified medium, the length scale  $\ell$  decreases with  $N$  and the maximum  $\ell_{\text{max}}$  is given by

$$\ell < \ell_{\text{max}} = c e^{1/2} N^{-1}, \quad c = 0.76. \quad (3b)$$

This translates into the limitation

$$G_h \equiv (\tau N)^2 < 100, \quad (3c)$$

which ultimately gives

$$\text{Ri}_c = 0.52. \quad (3d)$$

Since (3d) is manifestly at odds with a host of measured data, one has ample reason to doubt the reliability of (3a). Specifically, we show that (3a) goes against the following facts.

- 1) Equation (3b) is Eq. (1.6a) of Deardorff (1976), but HG overlooked a critical fact. The constant  $c$  in (3b) is not a universal constant. It depends on  $\text{Ri}$  itself. In fact, the specific value  $c = 0.76$  was chosen by Deardorff to obtain a value of  $\text{Ri}_c = 0.2$ ; that is,

$$c = 0.76 \quad \text{only for } \text{Ri}_c = 0.2. \quad (4a)$$

A different value of  $\text{Ri}_c$  yields another  $c$ . Since Deardorff did not give the function  $c(\text{Ri})$ , but only one value of it, HG have arbitrarily assumed that  $c$  is independent of  $\text{Ri}$ .

The fact that

$$c = c(\text{Ri}) \quad (4b)$$

has indeed been recently shown (Canuto et al. 2004,

manuscript submitted to *J. Atmos. Sci.*, hereafter CCH2).

- 2) Hassid and Galperin overlooked an additional critical fact. Deardorff himself realized that the *decrease of  $\ell$  with  $N$*  implied by (3b) was at odds with some LES data he obtained two years earlier (Deardorff 1974, Fig. 19), which showed that  *$\ell$  increases with  $N$* .
- 3) An increase of  $\ell$  with  $N$  (rather than a decrease) was later confirmed by several authors (Moeng and Wyngaard 1989; Schmidt and Schumann 1989; Moeng and Sullivan 1994).
- 4) On these grounds, Schumann concluded that “the increase of  $\ell$  contradicts the expectation which form the basis of Deardorff’s (1976) proposal.”
- 5) The question of whether in a stably stratified medium  $\ell$  increases or decreases with  $N$  is related to the presence or not of external sources such as shear (Canuto and Minotti 1993; Cheng and Canuto 1994; Canuto and Cheng 1997; CCH2). In the first case,  $\ell$  decreases with  $N$  while in the second case,  $\ell$  increases with  $N$ . As an example of  $\ell$  increasing with  $N$  comes from the Dickey–Mellor (1980, hereafter DM) experiment of freely decaying turbulence in a stably stratified medium.
- 6) The conclusion is strengthened by LES results by Schumann (1990, 1991) who found that  $\ell$  “decreases with increasing importance of shear.” The corollary is that  $\ell$  will eventually increase when shear becomes negligible.

Even without considering a  $P = \epsilon$  model, in the presence of sources (e.g., shear) and sinks (e.g., stable stratification), one has that

$$\text{strong sources} \Rightarrow \text{Ri small}, \quad (5a)$$

$$\text{weak sources} \Rightarrow \text{Ri large}. \quad (5b)$$

Relations (5a) and (5b) suggest relations of the form:

$$\text{Small Ri: } \ell \sim N^{-a}, \quad \text{Large Ri: } \ell \sim N^b, \quad (5c)$$

where  $(a, b)$  are two positive constants. What must be realized and implemented is that when the sources of turbulence vanish (for example near the top of the PBL),  $\ell$  begins to increase with  $N$ , and Deardorff’s requirement that  $\ell$  decreases with  $N$  no longer applies. This has as yet unexplored consequences on the value of  $\text{Ri}_c$  and thus on the height of the Planetary boundary layer (PBL) and/or the depth of the ocean mixed layer.

#### 5. The limitation on $G_m$

Hassid and Galperin (2004) assert that CCH did not take into account the limitation imposed on  $\text{Ri}_c$  by  $G_m(\text{max})$  defined in Eq. (21c) of CCH or Eq. (4) of HG. This is factually incorrect. First, CCH stated very clearly after Eq. (23) that “It is important to check the consistency of (21c) with (23b).” Second, in Figs. 1 and 2,

CCH plotted  $G_m$  and  $G_m(\max)$  versus  $Ri$  to show that in the CCH model

$$G_m < G_m(\max) \quad (6)$$

for all  $Ri$ . Hassid and Galperin should have concluded that this is another advantage of the CCH model since it automatically satisfies the  $G_m(\max)$  criterion (6) they proposed in 1983 (Hassid and Galperin 1983).

## 6. Concerning $\bar{v}^2$ and $\bar{w}^2$

Mellor (1973) pointed out that  $\bar{v}^2 = \bar{w}^2$  [his Eqs. (35b) and (35c)] may be related to the parameterization of the pressure correlation [his Eq. (10)] as he wrote that “We now see that (35b,c) permits only equal values of  $\bar{v}^2$  and  $\bar{w}^2$ , whereas the data indicated more or less unequal values. Possibly, this is a defect in the isotropy assumption involved in (10), but, hopefully not a serious impediment to our ultimate goal.”

Mellor’s insight into this problem 30 years ago is impressive. MY82 reiterated the idea by saying that “The fact that  $v'$  and  $w'$  are equal is not supported by the data” and “the model could be complicated to permit  $v' \neq w'$ .” The CCH model allowed  $v' \neq w'$  by allowing  $\lambda_2 \neq \lambda_3$ , a change that introduced a small increase in algebraic complexity. In this regard, CCH is a step forward along the direction foreseen by Mellor (1973).

Besides, as stated in CCH, the nonisotropic pressure correlations are part of state-of-the-art turbulence closures, which have been widely used in turbulence theories and in engineering studies. We see no reason why the PBL community should not embrace them.

Hassid and Galperin misquoted the CCH statement. What was stated in CCH is that the model “offers an alternative that will be able to, at least partially, account for the difference between  $\bar{v}^2$  and  $\bar{w}^2$ , without resorting to adding wall terms to the pressure correlations.”

CCH did not claim that their model could do everything the complicated wall functions do, but that it is able to represent some portion of the difference between  $\bar{v}^2$  and  $\bar{w}^2$ . Launder et al.’s (1975) Table 1 quoted experimental data for the homogeneous shear flow and the near wall flow, both of which show that  $\bar{w}^2 < \bar{v}^2$ , although in the homogeneous flow, the difference is smaller.

Most higher-order turbulence models, including the MY model and the CCH model, use model “constants” while strictly speaking, the constants may vary with space and time. Near the surface, the flow is more like a boundary layer flow, and in the mid-PBL, the flow is closer to the homogeneous flow. When the constants are determined, some compromise is needed so that the models can be more flexible in general conditions.

In the literature, different authors (e.g., Launder et al. 1975; Speziale et al. 1991; Taulbee 1992) have suggested different values for  $\lambda_2$  and  $\lambda_3$  (which determine how much  $\bar{v}^2$  differs from  $\bar{w}^2$ ). Taulbee (1992) pointed

out that the values  $\lambda_2/\lambda_3 = 0-0.47$  are also in use. In general, the ratio is smaller than unity.

A useful improvement of the CCH model, as compared with previous models, is that it provides formulas that depend explicitly on a more complete set of  $\lambda$  values. This not only yields Eq. (1d), but also allows  $\lambda_{2,3}$  to be different, instead of arbitrarily imposing  $\lambda_2 = \lambda_3$ , as in many previous models. We do not claim that the  $\lambda$  values are carved in stone. We welcomed, for example, the effort of Kantha (2003) to improve the constants of the CCH model, although we stress that imposing  $\lambda_2 = \lambda_3$  is a setback that is obviously inconsistent with many data (including the data in MY82) and thus should and can be avoided.

## 7. About Figs. 7–8 and 9–10 of CCH

The following is to answer HG’s question as to why MY and CCH give similar results in Figs. 7–8 but not in Figs. 9–10. The key reason is that Figs. 7–8 refer to the surface layer only, where  $Ri$  is small and where MY and CCH models differ little. On the other hand, in Figs. 9–10, the full range of  $Ri$  from 0 to  $Ri_c$  occur and thus the difference between MY and CCH becomes apparent.

To answer HG’s question about how Figs. 9–10 of CCH were produced, we state the following.

- 1) The results presented in Figs. 9–10 are derived from numerical simulations of the whole PBL (i.e., by solving the dynamic equations presented in CCH, using the CCH and MY turbulence models), as well as from LES.
- 2) Figures 9–11 employ Eq. (5e) of CCH, which was not used in any other figure of CCH.

It has been common practice for the LES authors to plot  $1/\Phi_m$  and  $1/\Phi_h$  versus  $Ri$  all the way to the top of the PBL. For example, in his Fig. 6 (similar to CCH Figs. 9–10), Andren (1995) noted that “ $Ri$ -values above 0.2 correspond in the simulations to levels well above mid-PBL height.” Brown et al. (1994), referring to their Fig. 10 (similar to our Figs. 9–10), write, “We now present data from the whole of the boundary layer. . .” Mason (1994) also plotted similar figures. In Kosovic and Curry’s (2000) LES data, at  $z/z_i = 0.58$ ,  $Ri$  reaches 0.23. Even if one only wants to look at the mid-PBL around  $Ri = 0.2$ , the CCH model is still much better than the MY model. It may help to think in  $Ri$  space with a domain (0,1). The MY82 models compress the domain to (0,0.2) and not surprisingly, the functions  $\Phi_{m,h}$  get distorted.

Hassid and Galperin (2004) write that: “. . . in order to obtain reasonable results for stable stratification, CCH have to use Nakanishi’s length scale. . .” Actually, we tried both cases. Without Nakanishi’s length scale, the match to the data for stable stratification is not as good, but still reasonable, especially if one considers that the original Kansas data are scattered in the stable region.



Nakanishi's (2001) new length scale is obtained from LES data and probably there is a point to using it.

In any case, Fig. 7 of CCH significantly improves MY82 for  $\Phi_m$  in the unstable region.

## 8. About the structural symmetry

The statement by HG that the structural symmetry exhibited by the solutions of CCH was not in the original equations misses our point. Two examples will suffice to make this clear. Newton's equations do not exhibit elliptical orbits as their solutions do! Dirac's equations for the relativistic electron does not exhibit the symmetry between matter and antimatter that only results from solving those equations. Neither is it visible in those equations that the electron has a spin, as the solutions show. And so on and so forth. In conclusion, there is a good reason why people solve equations, the results exhibit much more information and/or symmetry than the original equations.

## 9. Conclusions

Concerning HG's concluding remarks, CCH recognized and acknowledged the success of MY's pioneering work (see introduction of CCH paper). It was also MY themselves who first pointed out some of the model deficiencies, for example, the  $\bar{v}^2$  versus  $\bar{w}^2$  issue. CCH tried to improve the modeling pioneered by MY (thus the title of the CCH paper). Hassid and Galperin's complacency about the classical MY82 model contrasts MY's caveats decades ago.

Hassid and Galperin (2004) assert that the CCH model tunes second-moment closure models to specific flows. The opposite is true. For example, the models that impose  $\lambda_2 = \lambda_3$  actually tune the closure to the homogeneous case and result in  $\bar{v}^2 = \bar{w}^2$ , which is inconsistent with the data, as first noticed by Mellor (1973). On the other hand, CCH model employs state-of-the-art turbulence closure that permits  $\bar{v}^2 \neq \bar{w}^2$  (among other things), and thus is more general.

In summary, the CCH model yields order unity  $Ri_c$ , as demanded by a host of data, while the HG scheme fails to do so.

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